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# Random coefficients models of arms imports\*

Ron P. Smith

Birkbeck College, University of London

Ali Tasiran

Middlesex University and Göteborg University

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## Abstract

Prepared for Special Issue of *Economic Modelling* in honor of P.A.V.B. Swamy.

This paper uses a large panel of data with up to 19 time-series observations for almost 150 countries to estimate models of arms imports. Qualitative evidence suggest a non-linear relationship. As income and military expenditure grow, the propensity to import first rises and then falls as a domestic arms industry develops. We face the difficulty that there is virtually no data on domestic arms procurement or production capability. We try to avoid this difficulty by adopting a random coefficient approach in order to identify any systematic influences on import propensity, through the impact of military expenditure, size of the armed forces or income on unobserved domestic production capability. While a clear non-linear pattern is apparent in the cross-section relationship, once one allows for parameter heterogeneity such a pattern is not apparent in the time-series.

J.E.L. Classification: C23, C24, D74

Keywords: Sample selection, arms imports, random parameters.

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# 1 Introduction

The random coefficient model, RCM introduced by Swamy (1970), has been widely used in panels where one has data over units, such as countries,  $i = 1, 2, \dots, N$ , for a fairly large number of time periods  $t = 1, 2, \dots, T$ . The RCM estimator can be calculated as a weighted average of the coefficients estimated for each unit and has the advantage that it introduces a focus on coefficient heterogeneity at the initial stage of modelling. The empirical experience with large  $T$  panels is that the dispersion of coefficient estimates over units is not only large, but implausibly large. Baltagi and Griffin (1997), among many others, note this and the fact that pooled or averaged estimators (such as the Swamy RCM or the mean group estimator of Pesaran and Smith (1995)) tend to be much more reasonable. Similarly, using a panel of  $N = 57$  countries with  $T = 31$  annual observations Boyd and Smith (2002) estimate purchasing power parity equations where one would expect the elasticity of the exchange rate to price differentials to be close to unity. They find a range from -0.40 to 2.47 in static levels regression and from -2.21 to 7.93 for long-run estimates in a first order dynamic model. The dispersion is not reflected in measured sampling variability and the range is not substantially reduced by removing insignificant coefficients or shrinking the coefficients toward the mean. Boyd and Smith interpret the heterogeneity in terms of omitted variables, unobserved factors which bias the coefficient for any particular unit. However, since the correlation between the included and omitted variables is not structural, it averages to zero over time and units, producing reasonable estimates for the average effect.

Omitted variables are only one possible form of misspecification. As Swamy has emphasised in subsequent work, e.g. in Hall et al, (2009), there are also measurement errors, endogeneity problems, structural breaks and non-linearities which could cause coefficients to differ over countries. The effect of the non-linearity, which will be one focus of our concern, is that different units, observed at different values of the explanatory variables, provide different linear approximations to an underlying non-linear function. The procedure Hall et al. (2009) suggest ‘is to first estimate a model with coefficients that are allowed to vary as a result of the fundamental misspecifications in the model, and, then, to identify the specification biases that are occurring in the underlying coefficients and remove them.’ They focus on coefficient instability over time, in this paper we follow a similar procedure, but focus on coefficient instability over units in a large panel.

Misspecification seems likely in the example we examine. The aim is to explain arms imports in a large panel of countries as a function of the countries military expenditures and other variables. The data are very ‘noisy’, with missing values, zeros and severe problems of measurement error, non-linearity and omitted variables. The data are bad partly because there are strong incentives, both on the demand and the supply side, to misreport weapons transfers and many transfers, particularly of small arms and light weapons are illicit and unrecorded. Even where there is no intention to deceive, there are problems in defining arms imports, particularly for dual use items that can have military or civil uses. For instance, Iceland has no armed forces or military expenditures but has recorded arms imports, because suppliers regard some of the equipment supplied, e.g. for the coast guard, as military. Contracts can be complex involving spares, training and facilities as well as the systems themselves; there is often little information on prices or payments which may involve bribes and other corrupt practices and counter-trade (barter). The contracts often include offsets, promises by the exporter to set up production facilities in the importing country. In the estimated equations, there are almost certainly omitted explanatory variables such as domestic arms production capability and geostrategic factors, both of which are difficult to measure.

There is also likely to be a fundamental non-linearity in the relationship. Poor countries tend not to import major weapons systems, since they cannot afford them and the conflicts they are involved in, primarily civil wars, usually involve small arms, which are of low value and often domestically produced. Donors and international financial institutions also disapprove of expensive arms imports by poor countries.<sup>1</sup> As countries become richer and spend more on the military they import more major weapons systems, but beyond a certain size they are likely to establish a domestic arms industry which they protect for strategic reasons, thus reducing imports. The US, the biggest military spender, imports relatively little. Thus one might expect an inverted U shape relationship, as the elasticity of arms imports rises and then falls with the level of military expenditure or income. While one might expect non-linearity, previous work has not revealed a clear pattern. The non-linearity is apparent in cross-section estimates, e.g. in Levine et al. (1998), but not in the panels examined in our earlier paper, Smith and Tasiran (2005), referred

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<sup>1</sup>The negative response by the World Bank and others to the Tanzanian purchase of an expensive UK military air traffic control system from BAE is illustrative in this respect.

to as ST below.

When confronted with such noisy panels, applied investigators have a number of choices in selecting the samples used for estimation. One route is to choose to ignore some of the data, working with a balanced panel using a smaller sample of better quality data. A second route is to use all the data, imputing observations for missing values; and trying to allow for the sample selection bias that comes from ignoring missing or zero values and the coefficient heterogeneity that comes from misspecification. In ST, we chose the first route, considering a small balanced panel of better quality data with 19 time-series observations for each of 52 countries, ignoring the data for almost a hundred other countries. This paper examines the implications of choosing the second route: using all the data, trying to model the coefficient heterogeneity, and investigating the possible non-linearity within a random coefficient framework.

In Section 2, we discuss the random coefficient models. In section 3 we provide some background on the arms trade and the two data sources, SIPRI and WMEAT. We have imputed data for cases where there are data from one source but not the other, details of how this is done is given in Tasiran and Smith (2009). We then adopt two quite different approaches to the data. In Section 4, we follow ST and estimate demand functions for arms imports on this larger data set allowing for possible sample selection bias, but assuming quite a lot of coefficient homogeneity. As long as country fixed effects are included in both the selection and regression equations, sample selection bias does not appear to be a serious problem. However, the results for the whole sample of countries are rather different from those from the smaller balanced sample of countries. In section 5 we estimate simpler random coefficient models allowing for considerable coefficient heterogeneity to explore the extent to which the differences in coefficients can be explained by non-linearity. Section 6 contains some concluding remarks.

## 2 Random Coefficient Models

Hisao and Pesaran (2008) provide a review of random coefficients models, here we consider some aspects that are relevant for our investigation. Consider a heterogeneous panel model:

$$\mathbf{y}_i = \mathbf{W}_i \boldsymbol{\delta}_i + \mathbf{u}_i \quad (1)$$

where  $\mathbf{y}_i$  is a  $T \times 1$  vector, and  $\mathbf{W}_i$  is a  $T \times k$  vector of strictly exogenous variables, including the intercept and assume, that  $\boldsymbol{\delta}_i = \boldsymbol{\delta} + \boldsymbol{\eta}_i$  where  $E(\boldsymbol{\eta}_i) = 0$  and  $E(\boldsymbol{\eta}_i \boldsymbol{\eta}_j') = \boldsymbol{\Omega}$ , if  $i = j$ ,  $E(\boldsymbol{\eta}_i \boldsymbol{\eta}_j') = 0$  otherwise, and that the  $\boldsymbol{\eta}_i$  are independent of  $\mathbf{W}_i$ . As Pesaran, Haque and Sharma (2000) emphasise this assumption of the independence of the randomly varying parameters from the regressors is crucial and we return to it.<sup>2</sup> There are a large number of estimators for  $\boldsymbol{\delta} \equiv \mathbf{E}(\boldsymbol{\delta}_i)$ , the expected value of the random coefficients. The simplest is to compute the OLS estimates for each group<sup>3</sup>:

$$\hat{\boldsymbol{\delta}}_i = (\mathbf{W}_i' \mathbf{W}_i)^{-1} \mathbf{W}_i' \mathbf{y}_i \quad (2)$$

and then construct the average  $\bar{\boldsymbol{\delta}} = \sum_i \hat{\boldsymbol{\delta}}_i / N$ , estimating the  $k \times k$  covariance matrix  $\boldsymbol{\Omega}$  by

$$\hat{\boldsymbol{\Omega}} = \sum_i (\hat{\boldsymbol{\delta}}_i - \bar{\boldsymbol{\delta}})(\hat{\boldsymbol{\delta}}_i - \bar{\boldsymbol{\delta}})' / (N - 1) \quad (3)$$

Pesaran and Smith (1995) call  $\bar{\boldsymbol{\delta}}$  the Mean Group, MG, estimator. Its estimated covariance matrix is  $V(\bar{\boldsymbol{\delta}}) = \hat{\boldsymbol{\Omega}} / N$ .

Swamy (1970) suggests a feasible generalised least squares, GLS, estimator, which is equivalent to using a weighted average of the individual OLS estimates  $\hat{\boldsymbol{\delta}}_i$  instead of the MG unweighted average. Using the residuals and the unbiased estimate of the variance

$$\hat{\mathbf{u}}_i = \mathbf{y}_i - \mathbf{W}_i \hat{\boldsymbol{\delta}}_i; \quad s_i^2 = \hat{\mathbf{u}}_i' \hat{\mathbf{u}}_i / (T - k),$$

respectively, the estimated covariance of  $\hat{\boldsymbol{\delta}}_i$  is

$$V(\hat{\boldsymbol{\delta}}_i) = s_i^2 (\mathbf{W}_i' \mathbf{W}_i)^{-1}.$$

Swamy suggests estimating  $\boldsymbol{\Omega}$  by the unbiased estimator

$$\tilde{\boldsymbol{\Omega}} = \hat{\boldsymbol{\Omega}} - \sum_i V(\hat{\boldsymbol{\delta}}_i) / N. \quad (4)$$

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<sup>2</sup>In the static models considered in this paper, the assumption of independence ensures that all the estimators considered are unbiased. This result does not carry over to dynamic models as emphasised by Pesaran and Smith (1995).

<sup>3</sup>There are also a range of simulation based, parameteric, random coefficient models which involve assuming some distribution for the coefficients but do not require estimating the coefficient for each group. We do not consider these, since we have little prior knowledge of the appropriate distribution in our case.

However,  $\tilde{\Omega}$  need not be positive definite, and in practice it often is not. In this case, Swamy suggests setting the last term to zero and using  $\hat{\Omega}$ , from (3) as the estimator instead. Notice that although  $\tilde{\Omega}$  ignores the correction for the sampling error of  $\hat{\delta}_i$ , it is consistent as  $T$  goes to infinity, since the second term in  $\tilde{\Omega}$  goes to zero. However, the fact that  $\tilde{\Omega}$  is often not positive definite indicates that the second term can be very large for small  $T$ .  $\tilde{\Omega}$  is also a non-parametric estimator, since it just uses the distribution of the estimated  $\hat{\delta}_i$ , so is robust to any serial correlation or heteroskedasticity in the  $\mathbf{u}_i$  which would cause the usual estimator of  $V(\hat{\delta}_i)$  to be inconsistent.  $V(\hat{\delta}_i)$  measures only sampling variability and not possible misspecification, so may also under-estimate the uncertainty associated with  $\hat{\delta}_i$ .

The Swamy estimator of the mean is

$$\begin{aligned}\tilde{\delta} &= \sum_i D_i \hat{\delta}_i; \quad V(\tilde{\delta}) = \left\{ \sum_i \Psi_i^{-1} \right\}^{-1} \\ D_i &= \left\{ \sum_i [\Psi_i]^{-1} \right\}^{-1} [\Psi_i]^{-1}, \quad \Psi_i = \tilde{\Omega} + V(\hat{\delta}_i).\end{aligned}\tag{5}$$

The predictions of the individual coefficients can be improved by shrinking the OLS estimates towards the overall estimate:

$$\begin{aligned}\tilde{\delta}_i &= \mathbf{Q} \hat{\delta}_i + (I - \mathbf{Q}) \tilde{\delta} \\ \mathbf{Q} &= \left[ \tilde{\Omega}^{-1} + V(\hat{\delta}_i)^{-1} \right]^{-1} \tilde{\Omega}^{-1}\end{aligned}\tag{6}$$

The Swamy RCM can be interpreted either as a GLS estimator or an empirical Bayes estimator. Hsiao Pesaran and Tahmiscioglu (1999) review a variety of Bayes and empirical Bayes estimators of this sort and show that the MG and the Swamy estimator are asymptotically equivalent and have a standard asymptotic normal distribution for large  $N$  and  $T$  as long as  $\sqrt{N}/T \rightarrow 0$  as both  $N$  and  $T \rightarrow \infty$ . However, the MG estimator is unlikely to perform well when  $N$  or  $T$  are small since it is very sensitive to outliers which are a common feature of the group specific estimates in many applications. In principle, the weighting which the Swamy estimator applies, should reduce this problem; in practice, it may not, because the misspecification may cause the outlying estimate to be very precisely estimated and given a large weight.

Swamy suggested a Wald test for the homogeneity hypothesis  $H_0 : \delta_i = \delta$

$$S = \sum_i \left( \hat{\delta}_i - \delta_* \right)' V \left( \hat{\delta}_i \right)^{-1} \left( \hat{\delta}_i - \delta_* \right) \sim \chi^2 (k(N-1)); \quad (7)$$

where

$$\delta_* = \left[ \sum_i V \left( \hat{\delta}_i \right)^{-1} \right]^{-1} V \left( \hat{\delta}_i \right)^{-1} \hat{\delta}_i$$

is a weighted fixed effect estimator. This test is not appropriate when  $N$  is large relative to  $T$  and Pesaran and Yamagata (2008), PY, propose a variant of this test which has good properties when  $N$  is large relative to  $T$ . The problem with (7) arises because as  $N$  increases the number of estimated variances increases. Rather than using  $s_i^2 = \hat{\mathbf{u}}_i' \hat{\mathbf{u}}_i / (T - k)$ , calculated using  $\hat{\mathbf{u}}_i$  the residuals from the individual regressions the PY test uses  $\tilde{\mathbf{u}}_i = \mathbf{y}_i - \mathbf{W}_i \hat{\delta}_{FE}$  in calculating an estimate of  $\sigma_i^2$  which is used both in the test statistic and the weighted fixed effect estimator. Although this may appear a slight adjustment, it makes a large difference when  $N$  is large relative to  $T$ .

The focus in panels has traditionally been on the correlation of intercepts with regressors, but the problem can arise with slope heterogeneity even when intercept heterogeneity is allowed for. Consider a very simple example, where:

$$y_{it} = \alpha_i + \beta_i x_{it} + \varepsilon_{it}, \quad i = 1, \dots, N, t = 1, \dots, T. \quad (8)$$

If one imposes homogeneity of  $\beta_i$  on (8), e.g. the fixed effects model, and defines  $\eta_i = \beta_i - \beta$ , one gets

$$y_{it} = \alpha_i' + \beta x_{it} + (\eta_i x_{it} + \varepsilon_{it}),$$

One will get inconsistent estimation of  $\beta = E(\beta_i)$  if  $\eta_i x_{it}$  is correlated with  $x_{it}$ , independence requires that  $\eta_i$  is uncorrelated with any functions of  $x_{it}$ . In addition any variable correlated with  $\eta_i x_{it}$  can appear significant, when added to this regression. Averaging, rather than imposing homogeneity, avoids this problem.

In some cases, the heterogeneity in the parameters may stem from observed individual-specific characteristics which do not vary over time, say  $z_i$ .

$$\beta_i = \beta_0 + \beta_1 z_i + \eta_i,$$

or

$$y_{it} = \alpha_i + \beta_0 x_{it} + \beta_1 z_i x_{it} + \eta_i x_{it} + \varepsilon_{it}, \quad (9)$$



In cases where  $z_i$  is correlated with  $x_{it}$ , then  $z_i x_{it}$  is correlated with  $x_{it}^2$ , and it may be very difficult to distinguish (9) from an apparently non-linear model

$$y_{it} = \alpha_i + \beta_0 x_{it} + \beta_1 x_{it}^2 + u_{it}. \quad (10)$$

These two models have quite different predictions for the effect of a change in  $x_{it}$ , either just  $\beta_0$ , or  $(\beta_0 + 2\beta_1 x_{it})$ . This is a specific case of the more general issue as to how one distinguishes the effect of variations over time from the effect of variations over units.

This problem of significant but spurious non-linearity is discussed in more detail by Pesaran, Haque and Sharma (2000). In our case, one issue is whether or not we can determine if the apparent non-linearity in the cross-section pattern of weapons imports is spurious.

### 3 Context and data

The arms trade, although small in economic terms, about \$40bn a year, is a sensitive policy issue. Most countries have control systems that regulate exports of weapons; there are a range of international control measures including UN embargoes on the supply of weapons to particular countries; there are a range of supplier groups to control transfers for conventional arms (the Wassenaar arrangement), nuclear technology, chemical and biological weapons and missile technology. General reviews of the economics of the arms trade can be found in Garcia-Alonso and Levine (2007) and Brauer (2007). The decision by a country to import arms reflects threats, proxied here by its military expenditure; ability to pay, proxied by per capita income; the labour intensity of its force structure, proxied by armed forces per capita; and its domestic weapons production capability, for which there is no obvious proxy. As noted above, there is likely to be a non-linearity, since as demand for weapons increases, the large fixed costs in establishing domestic production capability are incurred and these substitute for imports. Establishing production capability takes time, so when weapons are urgently needed in a crisis, they tend to come from imports. Explaining arms imports is more difficult than explaining military expenditure because the data are very lumpy, imports can be small in one year, large in the next as say a squadron of aircraft are delivered and go back to being small. There are also a lot of zeros in the full data, where countries did not import at all or the imports were not measured. Questions of dynamic adjustment are

potentially important and these are discussed in Smith and Tasiran (2005). Allowing for slow adjustment did not change the main conclusions there so, given the short time-series dimension, we will confine our attention to static models here, though there remains the danger of dynamic misspecification. There are also obvious issues of endogeneity; since imports, military expenditures and armed forces may be determined simultaneously in the light of threats. However, given the lack of suitable instruments these issues will not be pursued.

There are two main sources of comparable international data on arms imports. The Stockholm International Peace Research Institute, SIPRI, regularly publish estimates in their annual yearbook as did the US government in the annual World Military Expenditure and arms Transfers, WMEAT, produced initially by the US Arms Control and Disarmament Agency and then by the Bureau of Verification and Compliance of the US Department of State. It has not been published since 2000, hence the end of the sample period that we use below. SIPRI attempts to measure volume by multiplying an estimate of quantities of major weapons systems transferred by trend indicator values of unit production costs, irrespective of the price actually paid. Thus when Germany transferred most of the old East German navy to Indonesia virtually free, this is still a quantity of weapons transferred and will be reflected as such by the SIPRI measure. WMEAT attempts to measure the value of arms transfers, reflecting an estimate of the price actually paid. There are thus two differences between the series: valuation (SIPRI is a quantity measure WMEAT a value measure) and coverage (SIPRI covers major weapons systems, WMEAT all systems).

In Smith and Tasiran (2005), we estimated dynamic demand functions for arms imports for a balanced panel of 52 countries and 19 years, 1981-1999, using cases where there were non-zero observations from both sources for all years. This approach ignores a large number of observations for countries where there are missing or zero observations for some years, though the observations excluded may be of lower quality. The missing observations may arise because countries disappear, e.g. the Soviet Union, and appear, e.g. Russia, neither of which were included in our balanced panel. This approach is also potentially subject to sample selection bias by not taking account of the zero observations, of which there are many in the whole data set. In this paper, we investigate the effect of using the whole data set, extending it by imputation where we have observations on arms imports from one of the two sources. We also investigate the effect of sample selection bias by examining

the effect of explicitly modelling the zeros.

One issue is whether arms imports are sensitive to price; the degree of price sensitivity is important for a range of policy issues. There are no direct measures of price, but since WMEAT is a value measure and SIPRI a volume measure, their ratio is an implicit price index. However, it is likely to be a poor measure and the implications for estimation of the measurement errors are discussed in ST, which considered a variety of estimators. We feel that even if the individual estimates of price are bad, they may allow us to estimate price sensitivity over the whole sample.

We have 2869 observations for 151 countries over 19 years on the dependent variable, though we lose some of these because we do not have data on covariates. As we extend the data set from the 988 (52x19) observations used in ST we gain more information but introduce more noise. We gain more information because the sample of 52 is likely to be biased, countries which have a full set of observations on arms imports may not be typical of the whole sample, and because we use the information in the zeros, which was ignored. We introduce more noise because the countries with less information are likely to have larger measurement errors and because the process of imputation introduces errors. As we extend the sample the estimated relationship may change either because more or worse data are being used or because the extra countries we include have a different process determining arms imports. We examine this by steadily increasing the sample and examining the effects on our estimates.

## 4 Homogeneous Demand Functions

We begin by treating the ratio of the WMEAT to the SIPRI series as a noisy measure of price and estimate demand functions, comparable with those in ST, of the form:

$$s_{it} = \alpha_i + \beta_1 p_{it} + \beta_2 m_{it} + \beta_3 ypc_{it} + \beta_4 afpc_{it} + u_{it}.$$

Where  $s_{it}$  is the logarithm of the SIPRI (quantity) measure of arms imports,  $p_{it}$  the logarithm of price (the ratio of the ACDA to the SIPRI measure),  $m_i$  is the logarithm of military expenditure,  $ypc_i$  the logarithm of per-capita income and  $afpc_{it}$  is the logarithm of armed forces per capita. This differs from the equation in ST by adding armed forces per capita; although its coefficient is not significant for the balanced panel, it is for the whole sample

and so the variable is included. The coefficients of the squares of  $m_{it}$  and  $ypc_{it}$  or their interaction are not significant in any of the samples so these variables are not included. We confine our attention in this section to the one way fixed effect estimator and allow for slope heterogeneity in the next section. All the standard errors reported are robust to heteroskedasticity, though not to serial correlation, for which there is some evidence.

We consider four samples. The first is the balanced sample using only countries where there are non-zero observations on the dependent variable for all years. This has 988 observations on 52 countries. The second, which we will call the extended sample, includes the non-zero observations for the countries excluded from the balanced panel because of missing observations. This has 1068 observations covering 94 countries. The third, pooled sample, combines these two, giving the whole sample excluding zeros, 2056 observations on 146 countries. The fourth, the whole sample, which includes the zeros, has 2454 observations on 150 countries. As one might expect the balanced sample is rather different from the extended sample, having higher average values for all the variables. Average log arms imports is 4.87 in the balanced sample compared to 2.62 in the extended; log military expenditure is 8.06 compared to 5.53; log income per capita is 8.42 compared to 7.23 and armed forces per capita 1.74 compared to 1.51. When estimating a linear relationship, if the data generating process is in fact linear, taking samples from different parts of the joint distribution does not matter. However if the underlying relationship is non linear, it does matter, since one gets different linear approximations in different parts of the joint distribution.

Table 1 gives the fixed effect estimates for the balanced sample, the extended sample and the pooled sample. The pooled estimates lie between the two subsamples, though since the coefficients are matrix weighted averages there is no requirement that they do so. The effects on the coefficients of log price are very small: the estimated price elasticities lie between -0.8 and -0.85. The coefficients of log military expenditure and log armed forces per-capita differ between samples, but do not change the qualitative story. The military expenditure elasticity rises from 0.54 on the balanced sample to 0.87 on the extended sample. The elasticity on armed forces per capita rises from 0.2 in the balanced panel where it is not significant to 0.45 in the extended sample. The coefficient on per-capita income is significantly positive in the balanced sample and significantly negative in the extended sample. The difference in the coefficient of  $afpc_{it}$  is not that large between the samples, but the larger sample reduces its standard error, so where it was excluded as insignificant

in ST, with the larger sample it is now significant. The data that were excluded from the balanced sample are clearly telling a rather different story from that in the balanced sample. The Likelihood ratio statistic for pooling is 98, which under the null of the same parameters would be  $\chi^2(4)$ , clearly rejects pooling the two samples. This test is valid under the assumption that the variances are the same. While the variances are significantly different, being rather larger in the extended sample the difference is not that large.

**Table 1 Fixed effect estimates of the demand function on different samples, robust standard errors in parentheses.**

	<i>NoObs</i>	<i>p<sub>it</sub></i>	<i>m<sub>it</sub></i>	<i>ypc<sub>it</sub></i>	<i>afpc<sub>it</sub></i>	<i>R<sup>2</sup></i>	<i>MLL</i>
	N					<i>SER</i>	
Balanced	988	−0.812	0.542	0.389	0.232	0.823	−1083.55
	52	(0.050)	(0.018)	(0.102)	(0.210)	0.746	
Extended	1068	−0.850	0.874	−0.452	0.454	0.824	−1425.98
	94	(0.037)	(0.034)	(0.083)	(0.091)	0.965	
Pooled	2056	−0.830	0.794	−0.128	0.315	0.864	−2558.51
	146	(0.027)	(0.030)	(0.062)	(0.068)	0.872	

**Notes.** *R<sup>2</sup>* is the coefficient of determination, *SER* the standard error of regression, *MLL* the maximised log-likelihood.

Including cases where the dependent variable is zero gives 2454 observations on 150 countries. Treating the zeros as if they were valid observations would be inappropriate, Wooldridge (2002, ch17) has a discussion of such sample selection models. Suppose that we have an underlying demand for the logarithm of the quantity (using the SIPRI measure) of arms imports,  $s_{it}^*$ , as a function of log offered price,  $p_{it}^*$  and

$$s_{it}^* = \alpha_i + \beta' \mathbf{m}_{it} - \gamma p_{it}^* + \varepsilon_{it}$$

where  $\mathbf{m}_{it}$  is a vector of explanatory variables including military expenditure and GDP. We only observe imports,  $s_{it}$  and prices,  $p_{it}$  if  $s_{it}^* > 0$ , i.e. if the reservation price is greater than the offered price

$$\gamma^{-1}(\alpha_i + \beta' \mathbf{m}_{it} + \varepsilon_{it}) > p_{it}^*.$$

This censors both the dependent variable and an independent variable, price and there is uncertainty as to whether  $s_{it} = 0$  indicates a true zero or a missing observation on a non-zero value.

We will consider selection models where different processes generate the zeros and non-zeros

$$\begin{aligned} z_{it}^* &= \mu_i + \delta' \mathbf{w}_{it} + \varepsilon_{1,it} \\ z_{it} &= \mathbf{1}(z_{it}^* > 0) \\ s_{it} &= \alpha_i + \beta' \mathbf{x}_{it} + (\varepsilon_{2,it} \mid z_{it} = 1) \end{aligned}$$

where  $\mathbf{1}(z_{it}^* > 0)$  is an indicator function which delivers 1 if the condition holds, zero otherwise. We only observe  $s_{it}$  and one element of  $\mathbf{x}_{it}$ , price, when  $z_{it} = 1$ , and  $\mathbf{w}_{it}$  is a vector of variables that we always observe. We assume that  $\varepsilon_{1,it}$  and  $\varepsilon_{2,it}$  are joint normal, with expected values zero. The variance of  $\varepsilon_{1,it}$  is normalised to unity, the variance of  $\varepsilon_{2,it}$  is  $\sigma^2$  and their correlation coefficient is  $\rho$ . The first equation is a Probit type selection equation determining whether arms imports are zero or not, the second selection corrected regression explains the non-zero observations. Tobit models are a special case where the same process generates the zeros and non-zeros, but this seems unlikely in this case: the factors determining whether or not to buy arms are likely to be different from the factors determining how many arms are purchased. As Dustmann and Rochina-Barachina (2007) note there are relatively few empirical studies using selection models in panels. We will use both random and fixed effect estimators for the  $\mu_i$  and  $\alpha_i$ . With the linear panel models above, Hausman tests strongly rejected the hypothesis that the intercepts are random, so there is a presumption in favour of the fixed effects model. In the fixed effect model the log-likelihood function for country  $i$  is

$$\begin{aligned} \log L_i &= \sum_{z_{it}=0} \log \Phi(-\mu_i - \delta' \mathbf{w}_{it}) \\ &+ \sum_{z_{it}=0} \left( \frac{-\log 2\pi}{2} - \log \sigma - \frac{(s_{it} - \alpha_i - \beta' \mathbf{x}_{it})^2}{2\sigma^2} \log \right) \\ &+ \sum_{z_{it}=0} \Phi \left[ \frac{(\mu_i + \delta' \mathbf{w}_{it}) + \frac{\rho}{\sigma}(s_{it} - \alpha_i - \beta' \mathbf{x}_{it})}{\sqrt{1 - \rho^2}} \right]. \end{aligned}$$

There is the difficulty that when  $z_{it} = 1$  for all the observations for a country, which is the case for the 52 countries in our balanced panel,  $\mu_i$  is not identified

in the probit fixed effect model. A similar problem occurs when  $z_{it} = 0$  for all observations for a country, though this does not arise in our data. In these cases the probit terms are just dropped from the Likelihood, it is assumed that there is a zero probability of no arms imports for these countries, which seems reasonable. The standard errors are bootstrapped<sup>4</sup>. For the probit selection equation it was found that the squares of log military expenditure and log per-capita income improved the fit and they were included.

**Table 2. Selection model estimates and (standard errors) for probit and selection corrected regression.**

	Random Effect		Fixed Effect	
$\mu$	3.829	(0.277)		
$m_{it}$	0.263	(0.061)	0.032	(0.025)
$m_{it}^2$	0.031	(0.007)	0.015	(0.027)
$ypc_{it}$	-1.37	(0.087)	-4.273	(1.31)
$ypc_{it}^2$	0.094	(0.006)	0.387	(0.095)
$afpc_{it}$	-0.009	(0.030)	0.479	(0.016)
$\alpha$	0.114	(0.092)		
$p_{it}$	-0.830	(0.007)	-0.830	(0.274)
$m_{it}$	0.570	(0.007)	0.780	(0.272)
$ypc_{it}$	0.002	(0.011)	-0.131	(0.152)
$afpc_{it}$	0.348	(0.014)	0.304	(0.109)
$\sigma$	0.878		0.841	
$\rho$	-0.153	(0.048)	-0.123	(0.103)

The estimates for the selection and regression equations are given in Table 2. In the random effect model  $\mu$  and  $\alpha$  are the means of the random  $\mu_i$  and  $\alpha_i$ . In this model the coefficient of armed forces is not significant in the selection equation and the coefficient of income not significant in the regression equation. In the fixed effects model the coefficient of military expenditure squared is not significant in the selection equation and the coefficient of income per capita in the regression equation. The estimate of  $\rho$  is similar and quite small in both, but significant only in the random effects model. In the regression equation, the random and fixed effects estimates

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<sup>4</sup>The estimation was done in the program Limdep which provides further details of estimation methods.

of the price elasticity are identical and the coefficients of armed forces quite close. However in the selection equation the fixed and random effects estimates are very different suggesting that the country intercepts are correlated with the regressors. The fixed effect regression estimates are very close to those given above for the pooled sample, which indicates that the correction for the missing zeros makes very little difference once one allows for fixed effects in the selection equation.

## 5 Heterogeneous Demand Functions

The previous section assumed considerable slope homogeneity, in this section this assumption is relaxed. To economise on parameters, we assume that the price elasticity is unity, then the log of the WMEAT measure of expenditure on arms imports,  $a_{it}$  can be made a function of military expenditure, both measured in 1999 US dollars, and of armed forces per capita. Income per-capita was not significant in any of the heterogeneous models, though this may be because of averaging negative and positive effects. Separate regressions are estimated for each of 131 countries where there were a minimum of six observations, 2228 observations in all, not using  $s_{it}$  increases the observations available.

To illustrate the issue of non-linearity, consider the cross-section, ‘between’ estimator, where the squares of both military expenditure and per-capita income are included. The estimates (with robust standard errors) are

$$\begin{array}{rcccccc}
 a_{it} = & 3.066 & +1.260 & m_{it} & -0.037 & m_{it}^2 \\
 & (1.56) & (0.19) & & (0.01) & \\
 & -1.345 & ypc_{it} & +0.08 & ypc_{it}^2 & +0.268 & afpc_{it} \\
 & (0.45) & & (0.03) & & (0.09) & 
 \end{array}$$

$R^2 = 0.837, SER = 0.78, N = 131$ .

The significant quadratic terms in the cross-section seem to indicate non-linearity. When the same equation is estimated by fixed effects none of the coefficients are significant, using robust standard errors; though all but military expenditure are significant using conventional standard errors. The fixed effects estimates of the pattern of non-linearity for military expenditure is the reverse of the cross-section, with  $m_{it}$  having a negative coefficient and  $m_{it}^2$  a positive one; though the coefficients of income were similar and



$afpc_{it}$  had a rather larger positive effect. The within group correlation of the variables seems to make it impossible to determine any non-linearities. Dropping the squared terms in the fixed effect estimator leads to  $m_{it}$  and  $afpc_{it}$  being significant, but not  $ypc_{it}$ .

To examine the effect of the treatment of coefficient heterogeneity, various versions of the simple model

$$a_{it} = \alpha_i + \beta_i m_{it} + \gamma_i afpc_{it} + u_{it} \quad (11)$$

were estimated using the cross-section, pooled OLS, one way fixed effects, FE1, two-way fixed effects FE2, Swamy RCM and mean group estimators.

**Table 3. Alternative estimates of (11)**

	$m_{it}$	( $se$ )	$afpc_{it}$	( $se$ )
<i>CS</i>	0.689	(0.045)	0.220	(0.089)
<i>Pool</i>	0.710	(0.014)	0.252	(0.032)
<i>FE1</i>	0.976	(0.076)	0.690	(0.048)
<i>FE2</i>	1.051	(0.060)	0.479	(0.079)
<i>RCM</i>	0.924	(0.200)	0.374	(0.250)
<i>MG</i>	1.008	(0.232)	0.306	(0.310)

Allowing for time effects, reduces  $\gamma$ , the coefficient of  $afpc_{it}$ , somewhat, but has little effect on the coefficient of military expenditure,  $\beta$ . The MG, FE and RCM estimates of  $\beta$  are very similar, with the between and pooled estimates somewhat lower. The two-way FE, MG and RCM estimates of  $\gamma$  are similar and not too far from the between and pooled, the one-way fixed effect estimate is rather higher. The RCM estimates have smaller standard errors than the MG estimates.

The RCM and MG estimates are averages of country specific coefficients. Table 4 provides descriptive statistics for the distribution of the 131 individual country estimates. Although all the distributions of the coefficients are broadly unimodal they have fat tails and normality is massively rejected by a skewness-kurtosis test. The strong skew of  $\hat{\alpha}_i$  results from some very negative outliers. One would expect the mean of  $\beta_i$  to be about unity: imports are a constant share of military expenditure; the range from -9 to +13 seems quite implausible. A more reasonable possible range for  $\beta_i$  might be -1 to +3 and 91 of the 131 (69%) estimates  $\hat{\beta}_i$  lie in this range. This is rather

more than the 58% one would expect with a normal distribution, given the estimated standard deviation. The outliers are not based just on small samples. If one only considers estimates based on 15 observations or more, the mean hardly changes and the standard deviation only reduces from 2.65 to 2.42. If one selects on significance, and only considers the 36 observations which are significantly positive at the 5% level, the mean increases to 3.47 with standard deviation 2.99. The range is from 0.5 to 12. Truncating on the basis of significance gives a very misleading picture, assuming that the true mean is around unity. Shrinking the estimates using the Swamy predictions of the coefficients, from (6), helps a little, reducing the range for  $\beta_i$  to from -2.08 to 9.06, but this is still larger than any reasonable range.

**Table 4. Descriptive statistics for the distribution of the coefficients, N=131**

	$\hat{\beta}_i$	$\hat{\gamma}_i$	$\hat{\alpha}_i$
Mean	1.008	0.306	-4.781
SD	2.65	3.55	24.51
Min	-8.58	-9.60	-183.32
Max	12.62	21.79	42.85
Skew	1.15	2.02	-4.03
Kurt	8.50	15.00	26.98
$\chi^2(2)$	193	874	3493

To examine whether there was any systematic relationship between the variables and the coefficients, Table 5 gives the correlations for the means of the variables and the coefficients. Although not an efficient way to estimate such relationships, it can sometimes be a useful diagnostic. The means of the variables are highly correlated, all greater than 0.49, in particular between average arms imports and average military expenditure as one would expect. The correlations between the estimates and the variables are all much lower. The strongest relationships are those with average military expenditure:  $-0.33$  for the intercept and  $+0.32$  for the coefficient of military expenditure. This positive relationship between  $\hat{\beta}_i$  and  $\bar{m}_i$  is not consistent with the inverted U shape indicated by the cross-section, which would lead one to expect a negative correlation. The coefficients of military expenditure have a strong negative correlation with the intercept and a weak negative relationship with the coefficient of armed forces.

**Table 5. Correlation matrix, country means and coefficients, N=131**

	$\bar{a}_i$	$\bar{m}_i$	$\overline{ypc}_i$	$\overline{afpc}_i$	$\widehat{\beta}_i$	$\widehat{\gamma}_i$	$\widehat{\alpha}_i$
$\bar{a}_i$	1						
$\bar{m}_i$	0.89	1					
$\overline{ypc}_i$	0.53	0.66	1				
$\overline{afpc}_i$	0.52	0.49	0.49	1			
$\widehat{\beta}_i$	0.18	0.32	0.22	0.24	1		
$\widehat{\gamma}_i$	-0.03	-0.04	-0.12	0.02	-0.28	1	
$\widehat{\alpha}_i$	-0.15	-0.33	-0.20	-0.22	-0.88	-0.10	1

Other specifications of the equations and of the determinants of the individual coefficients, also provided no clear results. However, the results are quite sensitive to specification. For instance, given that over this short sample most of the series have strong trends, adding a time trend makes the coefficient of  $afpc_{it}$  insignificant. We also considered various non-parametric and quantile regression estimators to try and identify the relationship, with the same general conclusion: the non-linearity was apparent in the between country cross-sections but not in the within country time-series.

## 6 Conclusion

When a country increases its military budget, the money can be spent on personnel, the procurement of new weapons, or the operation of the existing stock of arms. The procurement can be either from domestic producers or foreign sources. This paper has tried to estimate the factors that determine the elasticity of arms imports to military expenditures. We face the difficulty that there is virtually no data on domestic arms procurement and domestic production capability is almost certainly a crucial factor in determining the import elasticity. We tried to avoid this difficulty by adopting a random coefficient approach to try to identify any systematic influences that levels of military expenditure, size of the armed forces or income may have on the elasticity. Qualitative evidence suggests that there might be a non-linear relationship: with growth in income and military expenditure causing the import propensity to rise and then fall as a domestic arms industry developed. Such a pattern is apparent in the cross-section relationship. However,

once one allows for heterogeneity such a pattern is not apparent. We considered a variety of homogeneous and heterogeneous models of the demand for arms imports and considered the effect of moving from a smaller balanced panel with no zero observations to a larger sample. In this case classic sample selection bias arising from the zeros being censored made little difference. However, selecting a balanced sample did make a lot of difference because it excluded the countries where the level of military expenditure and imports was low and where the relationship was very different from the higher spending countries included in the balanced sample. In the larger sample, the logarithm of armed forces per-capita, a measure of the capital intensity of military provision was a significant determinant, which it was not in the balanced sample.

The nature of the data set also means that there are trade-offs in deciding which aspects of the specification one wishes to emphasise and our approach neglected dynamics, given the short time series available for some countries. There clearly is a large amount of heterogeneity in the coefficients and adopting a random parameter approach has the advantage that it draws attention to the heterogeneity at the stage of model specification. In the introduction we discussed the suggestion of Hall et al. (2009) ‘to first estimate a model with coefficients that are allowed to vary as a result of the fundamental misspecifications in the model, and, then, to identify the specification biases that are occurring in the underlying coefficients and remove them.’ Unfortunately, it is often easier to identify the symptoms of misspecification than to remove them. The major indication of possible misspecification in our case was the extreme and implausible range of coefficients across countries, a common feature of heterogeneous panels. Allowing the coefficients to vary over countries, we tried to identify the reasons for the large differences in estimates between countries and to determine whether the differences could be explained by the non-linearity apparent in cross-section. While there was little indication of within country non-linearity, this may be because our short time-series did not contain enough within country variation in the explanatory variables for any non-linearity to become apparent. Longer time-series or data on domestic weapons procurement and production capability is probably necessary for any clear conclusion. Despite the lack of clear conclusion, the random parameter approach does seem a useful way to approach differences between time-series and cross-section patterns.

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